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PROCEEDINGS

OF

THE ROYAL IRISH ACADEMY.

1841. No. 31.

November 8.

SIR Wm. R. HAMILTON, LL.D., President, in the Chair.

John H. Jellett, Esq., F.T.C.D., was elected a member of the Academy.

A letter was read from Dr. Orpen, stating that increasing ill health would not allow him to continue to discharge his duties to the Academy, and tendering the resignation of his office of Treasurer, and of his place as a member of Council.

Resolved,—That the Academy have heard Dr. Orpen's communication with much regret, and that they deeply lament the cause which deprives them of his valuable services.

Professor Mac Cullagh read the following notes on some points in the Theory of Light.

I.

On a Mechanical Theory which has been proposed for the Explanation of the Phenomena of Circular Polarization in Liquids, and of Circular and Elliptic Polarization in Quartz or Rock-crystal; with Remarks on the corresponding Theory of Rectilinear Polarization.

The theory of elliptic polarization, which I feel myself called upon to notice, was first stated by M. Cauchy, and has been made the subject of elaborate investigation by other writers. That celebrated analyst, conceiving (though without sufficient reason, as will presently appear) that he had fully explained the known laws of the propagation of *rectilinear* vibrations by the hypothesis that the luminiferous ether, in media transmitting such vibrations, consists of separate molecules *symmetrically* arranged with respect to each of three rectangular planes, and acting on each other by forces which are some function of the distance, was led very naturally to imagine that he would find the laws of *circular* and *elliptic* vibrations, in other media, to be included in the more general hypothesis of an *unsymmetrical* arrangement. Accordingly, in a letter read to the French Academy on the 22nd of February, 1836, a letter to which he attached so much importance that he desired it might not only be published in the Proceedings, but also "deposited in the Archives" of that body (see the *Comptes rendus des Séances de l'Académie des Sciences*, tom. ii. p. 182), he gave a precise statement of his more extended views, informing the Academy that he had submitted his new theory to calculation, and that, among other remarkable results, he had obtained (with a slight variation or correction) the laws of circular polarization, discovered by Arago, Biot, and Fresnel. Referring to his Memoir on Dispersion, published at Prague, under the title of *Nouveaux Exercices de Mathématiques*, he observes, that the results therein contained may be generalised, by "ceasing to neglect" in the equations of motion [the equations marked (24) in § 2 of that memoir], certain terms which vanish in the case of a symmetrical distribution of the ether. He then goes on to say—

"Nos formules ainsi généralisées représentent les phénomènes de l'absorption de la lumière ou de certains rayons, produite par les verres colorés, la tourmaline, &c., le phenomène de la polarisation circulaire produite par le cristal de roche, l'huile de térébenthine, &c. (*Voir* les expériences de MM. Arago, Biot, Fresnel). Elles servent même à déterminer les conditions et les lois de ces phénomènes ; elles montrent que géneralement, dans un rayon de lumière polariseé, une molecule d'éther décrit une ellipse. Mais dans certains cas particuliers, cette ellipse se change en une droite, et alors on obtient la polarisation rectiligne." " Enfin le calcul prouve que, dans le cristal de roche, l'huile de térébenthine, &c., la polarisation des rayons transmis parallèlement à l'axe (s'il s'agit du cristal de roche) n'est pas rigoureusement circulaire, mais qu'alors l'ellipse diffère très peu du cercle."

Thus, to say nothing for the present of the questions of dispersion and absorption, it appears that M. Cauchy conceived he had completely accounted for the facts of circular and elliptic polarization, and that he had deduced the formulas " which serve to determine the conditions and laws of these phenomena." But neither in this letter, nor in any subsequent version* of his theory, has he given the formulas themselves. Nor has he told us the nature of the calculations by which he was enabled to correct the received opinion, and to prove that the vibrations in a ray transmitted along the axis of quartz, or through oil of turpentine, are not rigorously circular, as Fresnel and others have supposed, but slightly elliptical. Now—to take the case of quartz—if we consider that the vibrations of a ray passing along the axis are in a plane perpendicular to it, and if we admit, as M. Cauchy always does in the case of other uniaxal crystals, that there is a perfect optical symmetry all round the axis, we shall find it hard to conceive on what grounds he could have come to the conclusion that the vibrations of such a ray are performed in an ellipse. For if all planes passing through the axis of the crystal be alike in their optical properties, there will be absolutely nothing to determine the position and ratio of the axes of the ellipse; there will be no reason why its major axis, for example, should lie in one of these planes, rather than in any other. But, whatever may be thought of this case independently of observation, it is manifestly absurd to suppose that the vibrations are elliptical in the case of a ray passing through oil of turpentine, or any other *liquid* possessing the property of rotatory polarization; for, in a liquid, all planes drawn through the ray itself are circumstanced alike. From these simple considerations it is evident that the theory of M. Cauchy is unsound; but a closer examination will show that it is entirely without foundation, and that it is directly opposed to the very phenomena which it professes to explain. To make this appear, however, in the easiest way that the abstruseness of the subject will allow, it will be necessary to advert to some former researches of my own, which have a direct bearing on the question.

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* From some statements that have been made within the last few days by Professor Powell (Phil. Mag. vol. xix. p. 374), at the request of M. Cauchy himself, it appears that the latter republished his views about circular and elliptic polarization, in a lithographed memoir of the date of August, 1836. But I do not find that he published, either then or since, the detailed calculations which he seems to have made.

The same day on which M. Cauchy's letter was read to the French Academy, I had the honour of reading to the Royal Irish Academy, a paper "On the Laws of Double Refraction in Quartz" (see Transactions R. I. A., vol. xvii. p. 461), wherein I showed that every thing which we know respecting the action of that crystal upon light is comprised mathematically in the following equations:

$$\begin{aligned} \frac{d^2\xi}{dt^2} &= \mathrm{A}\frac{d^2\xi}{dz^2} + \mathrm{C}\frac{d^3\eta}{dz^3}, \\ \frac{d^2\eta}{dt^2} &= \mathrm{B}\frac{d^2\eta}{dz^2} - \mathrm{C}\frac{d^3\xi}{dz^3}, \end{aligned} \qquad (1)$$

which differ from the common equations of vibratory motion by the two additional terms containing third differential coefficients multiplied by the same constant C, this constant having *opposite* signs in the two equations. The quantities ξ and η are, at any time t, the displacements parallel to the axes of x and y, which are supposed to be the principal directions in the plane of the wave, one of them being therefore perpendicular to the axis of the crystal. The constants A and B are given by the expressions

$$\mathrm{A} = a^2, \quad \mathrm{B} = a^2 - (a^2 - b^2) \sin^2\psi,$$

where a and b are the principal velocities of propagation, ordinary and extraordinary, and ψ is the angle made by the wave-normal (or the direction of z) with the axis of the crystal. The only new constant introduced is C, which, though the peculiar phenomena of quartz depend entirely on its existence, is almost inconceivably small; its value is determined in the paper just referred to. The equations are there proved to afford a strict geometrical representation of the facts; not only connecting together all the laws discovered by the distinguished observers to whom M. Cauchy refers, and including the subsequent additions for which we are indebted to Mr. Airy, but leading to new results, one of which establishes a relation between two different classes of phenomena, and is verified by the experiments of M. Biot and Mr. Airy. Having, therefore, such conclusive proofs of the truth of these equations, we are entitled to assume them as a standard whereby to judge of any theory; so that any mechanical hypothesis which leads to results inconsistent with them may be at once rejected.

Now I assert that the mechanical hypothesis of M. Cauchy contradicts these equations, and therefore contradicts all the phenomena and experiments which he supposed it to represent. But before we proceed to the proof of this assertion, it may perhaps be proper to remark, that previously to the date of M. Cauchy's communication, and of my own paper, I had actually tried and rejected this identical hypothesis, and had even gone so far as to reject along with it the whole of M. Cauchy's views about the mechanism of light. For though, in my paper, I have said nothing of any mechanical investigations, yet, as a matter of course, before it was read to the Academy, I made every effort to connect my equations in some way with mechanical principles; and it was because I had failed in doing so to my own satisfaction, that I chose to publish the equations without comment,* as bare geometrical assumptions, and contented myself with stating orally to the Academy, as I did some months after to the Physical Section of the British Association in Bristol (see Transactions of the Sections, p. 18), that a mechanical account of the phenomena still remained a *desideratum* which no attempts of mine had been able to supply. I am not sure that on the first occasion I stated the precise nature of these attempts, though I incline to think I did; but I have a distinct recollection of having done so on the second occasion, in reply to questions that were asked me by some Members of the Association.† Now, my first attempt to explain those equations, which was made almost as soon as I discovered them, actually turned upon the very idea which about the same time found entrance into the mind of M. Cauchy—I mean the idea of an unsymmetrical arrangement of the ether. For as it was generally believed, at that period, that the hypothesis of ethereal molecules symmetrically distributed had led, in the hands of M. Cauchy, to a complete theory of rectilinear polarization in crystals (see his *Exercices de Mathématiques, Cinquième Année*, Paris, 1830, and the *Mémoires de l'Institut*, tom x. p. 293), the notion of endeavouring to account for the phenomena of elliptic polarization, by freeing the hypothesis from any restriction as to the distribution of the ether, would naturally occur to any one who was thinking on the subject, no less than to M. Cauchy himself. And though, for my own part, I never was satisfied with that theory, which seemed to me to possess no other merit than that of following out in detail the extremely curious, but (as I thought) very imperfect, analogy which had been perceived to exist between the vibrations of the luminiferous medium and those of a common elastic* solid (for it is usual to regard such a solid as a rigid system of attracting or repelling molecules, and M. Cauchy has really done nothing more than transfer to the luminiferous ether both the constitution of the solid and differential formulas of its vibration), still I should have been glad, in the absence of anything better, to find my equations supported by a similar theory, and their form at least countenanced by the like mechanical analogy. Besides, I recollected that Fresnel himself, in his Memoir on Double Refraction, had indicated a " helicoidal arrangement," or something of that sort, as a probable cause of circular polarization (*Mémoires de l'Institut*, tom. vii. p. 73); and as this was an hypothesis of the same kind as the other, only not so general, I was prepared to find that the supposition of an arbitrary arrangement, whatever might be thought of its physical reality, would lead to equations *of the same form* as those which I had assumed. Upon trial, however, the very contrary proved to be the case, for though it was possible to obtain additional terms, containing differential coefficients of the third order, multiplied by the same constant C, yet this constant always came out with the *same* sign in both equations, whereas a *difference* of sign was *essential* for the expression of the phenomena. I had no sooner arrived at this result, than I perceived it to be fatal to the theory of M. Cauchy, and to afford a demonstration of its insufficiency, not only in the particular application which I had made of it, but in all its applications. For the hypothesis which I used was, in fact, identical with that theory, in the most general form of which it is susceptible, when unrestricted by any particular supposition as to the arrangement of the ethereal molecules; and therefore the fundamental conception of the theory could not be true, as it not merely failed to explain a large and most remarkable class of phenomena—those of circular and elliptical polarization—but absolutely excluded them, and left no room for their existence. It followed from this, that the mechanical explanation, which the same theory was supposed to have given, of the phenomena of rectilinear polarization and double refraction in crystals, could not be well founded; indeed, as I have said, I had always distrusted it, and that for various reasons, of which one has been already mentioned, and another was suggested by the forced relations which M. Cauchy had found it necessary to establish among the constants of his theory, and by which he had *compelled*, as it were, his complicated formulas to assume the appearance of an agreement (though, after all, a very imperfect one) with the simple laws of Fresnel.

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* The circumstances here related will account for what Mr. Whewell (*History of the Inductive Sciences*, vol. ii. p. 449) calls the "obscure and oracular form" in which those equations were published. Having, at that time, no good explanation of them to give, I thought it better to attempt none. But in the general view which I have since taken (see p. 103 of this volume), they do not offer any peculiar difficulty.

† At the period of this meeting, M. Cauchy's letter on Elliptic Polarization had been published for some months; but I was not then aware of its existence. Indeed the letter appears not to have attracted any general notice; for the theory which it contains was afterwards advanced in England as a new one, and M. Cauchy has been lately obliged to assert his prior claim to it, through the medium of Professor Powell.—See notes, pp. 141, 149.

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* The analogy was suggested by the hypothesis of transversal vibrations, which, when viewed in its physical bearing, was considered by Dr. Young to be "perfectly *appalling* in its consequences," as it was only to *solids* that a "lateral resistance" tending to produce such vibrations had ever been attributed. (*Supplement to the Encyclopædia Britannica*, vol. vi. p. 862: Edinburgh, 1824). He, admits, however, that the question whether fluids may not "transmit impressions by lateral adhesion, remains completely open for discussion, notwithstanding the apparent difficulties attending it." As far as I am aware, Fresnel always regarded the ether as a *fluid*. M. Poisson affirms that it *must* be so regarded, and attributes its apparent peculiarities to the immense rapidity of its vibrations, which does not allow the law of equal pressure to hold good in the state of motion (*Annales de Chimie*, tom. xliv. p. 432). M. Cauchy calls the ether a fluid, though he treats it as a solid. My own impression is, that the ether is a medium of a peculiar kind, differing from all ponderable bodies, whether solid or fluid, in this respect, that it absolutely refuses, in any case, to change its density, and therefore propagates to a distance transversal vibrations *only*; while ordinary elastic fluids transmit only normal vibrations, and ordinary solids admit vibrations of both kinds. This hypothesis also includes the supposition that the density of the ether is unchanged by the presence of ponderable matter. As to M. Cauchy's *third ray*, with vibrations nearly normal to the wave, there is no reason to believe that it has even the *faintest* existence; but it is necessarily introduced by his identification of the vibrations of light with those of an indefinitely extended elastic solid.

Such were the conclusions at which I arrived, and the reflections which they forced upon me, nearly six years ago. They have been frequently mentioned in conversation to those who took an interest in such matters, and their general tenor may be gathered from what I have elsewhere written (Transactions of the Academy, vol. xviii. p. 68); but I did not think it worth while to publish them in detail, because it seemed probable that juster notions would prevail in the course of a few years, and that the ingenious speculations to which I have alluded would gradually come to be estimated at their proper value. But from whatever cause it has arisen—whether from the real difficulties of the subject, or the extreme vagueness of the ideas that most persons are content to form of it, or from deference to the authority of a distinguished mathematician—certain it is that the doctrines in question have not only been received without any expression of dissent, but have been eagerly adopted, both in this country and abroad, by a host of followers; and even the extraordinary error, which it is my more immediate object to expose, has been continually gaining ground up to the very moment at which I write, and has at last begun to be ranked among the elementary truths of the undulatory theory of light. Notwithstanding my unwillingness, therefore, to be at all concerned in such discussions, I do not think myself at liberty to remain silent any longer.

There are occasions on which every consideration of this kind must give way to a regard for the interests of science.

To show that the principles of M. Cauchy contradict, instead of explaining, the phenomenon of elliptic polarization, let us take the axes of coordinates as before; and let us suppose, for the sake of simplicity, and to avoid his *third ray*, that the normal displacements vanish. Then his fundamental equations take the form

$$\begin{aligned} \frac{d^2\xi}{dt^2} &= \Sigma f \Delta \xi + \Sigma h \Delta \eta, \\ \frac{d^2\eta}{dt^2} &= \Sigma g \Delta \eta + \Sigma h \Delta \xi, \end{aligned} \qquad (2)$$

where f, g, h are quantities depending on the law of force and the mutual distances of the molecules.* If, therefore,

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* I have not thought it necessary to transcribe the original equations of M. Cauchy, which are rather long. He has presented them in different forms; but the system marked (16) at the end of § 1 of his Memoir on Dispersion, already quoted, is the most convenient, and it is the one which I have here used. The directions of the coordinates being arbitrary, I have supposed the axis of z to be perpendicular to the wave-plane. Then, on putting $\zeta = 0$, $\Delta\,\zeta = 0$, in order to get rid of the normal vibration, the last equation of the system becomes useless, and the other two are reduced to the equations (2), given above; the letters f, g, h, being written in place of certain functions depending on the mutual actions of the molecules. It will be proved, further on, that this simplification does not at all affect the argument. As the directions of x and y still remain arbitrary, I have made them parallel to the axes of the supposed elliptic vibration.

It may be right to observe, for the sake of clearness, that, when the medium is arranged symmetrically, it is always possible to take the directions of x and y such that the two sums depending on the quantity h may disappear from the equations (2), and then the vibrations are rectilinear. But when the arrangement is unsymmetrical, this is no longer possible.

The equations (2) are precisely the same as those which have been employed by Mr. Tovey and by Professor Powell, the latter of whom, in his lately published work, entitled, "*A General and Elementary View of the Undulatory Theory, as applied to the Dispersion of Light, and other Subjects*," has dwelt at great length on the theory of elliptic polarization, which they have been supposed to afford, and which he regards as a most important accession to the Science of Light. Professor Powell has also made some communications on the subject to the British Assowe assume that each molecule describes an ellipse, the axes of which are parallel to those of x and y; that is to say, if we make

$$\xi = p\cos\phi, \quad \eta = q\sin\phi, \qquad \phi = \frac{2\pi}{\lambda}(st - z), \tag{3}$$

and consequently

$$\Delta\xi = p(\sin 2\theta\sin\phi - 2\sin^2\theta\cos\phi),$$
$$\Delta\eta = -q(\sin 2\theta\cos\phi + 2\sin^2\theta\sin\phi),$$

where $\theta = \frac{\pi\Delta z}{\lambda}$, we shall find, by substituting these values in the equations (2), which must hold good independently of ϕ,

$$s^2 = \mathrm{A}' + \mathrm{C}'k, \qquad s^2 = \mathrm{B}' - \frac{\mathrm{C}'}{k}, \tag{4}$$
$$\Sigma f\sin 2\theta - 2k\Sigma h\sin^2\theta = 0,$$
$$\Sigma g\sin 2\theta + \frac{2}{k}\Sigma h\sin^2\theta = 0,$$

wherein $k = \frac{q}{p}$ expresses the ratio of the semiaxes of the elliptic vibration, and

$$\mathrm{A}' = \frac{\lambda^2}{2\pi^2}\Sigma f\sin^2\theta, \qquad \mathrm{B}' = \frac{\lambda^2}{2\pi^2}\Sigma g\sin^2\theta,$$
$$\mathrm{C}' = \frac{\lambda^2}{4\pi^2}\Sigma h\sin 2\theta.$$

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ciation, and has written two papers about it in the Philosophical Transactions (1838, p. 253; and 1840, p. 157), besides several others in the Philosophical Magazine. He, however, always attributed this theory of elliptic polarization to Mr. Tovey, until his attention was directed, by a letter from M. Cauchy, to some investigations of the latter which he had not previously seen (Phil. Mag. vol. xix. p. 374). Mr. Tovey set out with the principles of M. Cauchy, and therefore naturally struck into the same track, in pursuit of the same object, apparently quite unconscious that any one had preceded him. It was, indeed, an obvious reflection, that these principles, when generalised to the utmost, ought to include, not only the laws of elliptic polarization, but (as really has been thought by M. Cauchy and his followers) of dispersion and absorption, and, in short, of all the phenomena of optics.

Equating the two values of s^2, we get, for the determination of k, the following quadratic:

$$k^2 + \frac{A' - B'}{C'} k + 1 = 0. \qquad (5)$$

Now making the substitutions (3) in equations (1), page 142, we have

$$s^2 = A - \frac{2\pi}{\lambda} Ck, \qquad s^2 = B - \frac{2\pi}{\lambda}\frac{C}{k}, \qquad (6)$$

and thence

$$k^2 - \frac{\lambda}{2\pi C}(A - B)\,k - 1 = 0, \qquad (7)$$

a result which is perfectly inconsistent with the former, since the two roots of (5) have the *same sign*, if they are not imaginary, while those of (7) have *opposite signs*, and cannot be imaginary. If, therefore, one equation agrees with the phenomena, the other must contradict them. The last equation indicates that, in the double refraction of quartz, the two elliptic vibrations are always *possible*, and performed in *opposite* directions, which is in accordance with the facts; whereas the equation (5), deduced from M. Cauchy's theory, would inform us that the vibrations of the two rays are either *impossible* or in the *same* direction.*

To apply the results to a particular instance, let us conceive a circularly polarized ray passing along the axis of quartz, or through one of the rotatory liquids, such as oil of turpentine; the position of the coordinates x and y, in the plane of the wave, being now, of course, arbitrary. In each of these cases we have $k = \pm 1$, and $A = B = a^2$, so that the value of s^2 in equation (6) is expressed by the constant a^2, *plus* or *minus* a term which is inversely proportional to the wave-length λ; the sign of this term depending on the direction of the circular vibration. Now it will not be possible to obtain a similar value of s^2 from the formulas (4), unless we suppose $\text{A}' = \text{B}' = a^2$, since it is only in the expansion of C' that a term inversely proportional to λ can be found; but on this supposition the formulas are inconsistent with each other, nor can they be reconciled by any value of k. Indeed, when $\text{A}' = \text{B}'$, the equation (5) gives $k = \pm \sqrt{-1}$. Thus it appears that circular vibrations, such as are known to be propagated along the axis of quartz, and through certain fluids, cannot possibly exist on the hypothesis of M. Cauchy. It was probably some partial perception of this fact that caused M. Cauchy to assert that the vibrations, in these cases, are not exactly circular, but in some degree elliptical; a supposition which, if it were at all conceivable, which we have seen it is not (p. 142), would be at once set aside by what has just been proved; for no assumed value of k, whether small or great, will in any way help to remove the difficulty.

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* This conclusion, which shows that M. Cauchy's Theory is in direct opposition to the phenomena, might have been obtained without any reference to the equations (1). But these equations are necessary in what follows.

But this is not all. Rectilinear vibrations are excluded as well as circular; for we cannot suppose $k = 0$ in the equations (4), so long as the quantity C', resulting from the hypothesis of unsymmetrical arrangement, has any existence. Thus the inconsistency of that hypothesis is complete, and the equations to which it leads are utterly devoid of meaning.

The foregoing investigation does not differ materially from that which I had recourse to in the beginning of the year 1836. To render the proof more easily intelligible, and to get rid of M. Cauchy's "third ray," which has no existence n the nature of things, I have suppressed the normal vibrations; a procedure which is not, in general, allowable on the principles of M. Cauchy. It will readily appear, however, that this simplification still leaves the demonstration perfectly rigorous in the case of circular vibrations, and does not affect its force when the vibrations are elliptical. For in the rotatory fluids it is obvious that the normal vibrations, supposing such to exist, must, by reason of the symmetry which the fluid constitution requires, be independent of the transversal vibrations, and separable from them, so that the one kind of vibrations may be supposed to vanish when we wish merely to determine the laws of the other. The equations (2) are, therefore, quite exact in this case; and they are also exact in the case of a ray passing along the axis of quartz, since such a ray is not experimentally distinguishable from one transmitted by a rotatory fluid, and its vibrations must consequently be subject to the same kind of symmetry. In these two cases, therefore, it is *rigorously* proved that the values of k, which ought to be equal to *plus* and *minus* unity, are imaginary, and equal to $\pm \sqrt{-1}$. And if we now take the most general case with regard to quartz, and suppose that the ray, which was at first coincident with the axis of the crystal, becomes gradually inclined to it, the values of k must evidently continue to be imaginary, until such an inclination has been attained that the two roots of equation (5) become possible and equal, in consequence of the increased magnitude of the co-efficient of the second term. Supposing the last term of that equation to remain unchanged, this would take place when the co-efficient of k (without regarding its sign) became equal to the number 2, and the values of k each equal to unity, both values being positive or both negative. The vibrations which before were impossible, would, at this inclination, suddenly become possible; they would be *circular*, which is the exclusive property of vibrations transmitted along the axis; and they would have the same direction in both rays, which is not a property of any vibrations that are known to exist. At greater inclinations the vibrations would be elliptical, but they would still have the same direction in the two rays. These results would not be sensibly altered by regarding the equation (5) as only approximate in the case of rays inclined to the axis; for the last term of that equation, if it does not remain the same, can never differ much from unity; since it must become exactly equal to unity, *whatever* be the direction of the ray, when the crystalline structure is supposed to disappear, and the medium to become a rotatory fluid.

That a theory involving so many inconsistencies should have been advanced by a person of M. Cauchy's reputation, would, perhaps, appear very extraordinary, if we did not recollect that it was unavoidably suggested by the general principles which he had previously adopted, and which were supposed, not merely by himself, but by the scientific world generally, to have already afforded the only satisfactory explanation of the laws of double refraction in the common and well-known case where the vibrations are rectilinear. This supposed explanation was obtained, as has been said, by restricting the application of M. Cauchy's principles to the hypothesis of a vibrating medium arranged symmetrically, in which case it was shown that the vibrations were necessarily rectilinear; and of course the removal of this restriction was the only way in which it was possible, on those principles, to account for the existence of circular and elliptical vibrations. Accordingly, when M. Cauchy perceived that, on the hypothesis of unsymmetrical arrangement, the existence of rectilinear vibrations became impossible, and that of elliptic vibrations, generally speaking, possible, he found it very easy to persuade himself that he had obtained a new proof of the correctness of his views, and a new and most important application of the fundamental equations by which his general principles were analytically expressed. To have supposed otherwise would have been to admit that his general principles were false. If the elliptical or *quasi*-circular vibrations which he was now contemplating were not capable of being identified with those which had been recognized in the phenomena presented by quartz and the rotatory fluids—if their laws were essentially or very considerably different—his theory would be inconsistent with a wide range of well known facts, and, notwithstanding its so-called explanations of other laws, should be finally abandoned. Under these circumstances, therefore, he very naturally supposed that his new results *must* be in complete harmony with the phenomena discovered by M. Arago, and analysed so successfully by MM. Biot and Fresnel; although, had he taken the precaution of acquiring such a clear notion of the phenomena as would have enabled him to translate them into analytical language, he must have perceived that they were entirely opposed to his results, and that this opposition furnished an argument which swept away the very foundations of his theory. For, if the constitution of the luminiferous medium were such as M. Cauchy supposes, the well-known phenomena of circular and elliptic polarization would, as we have seen, be absolutely impossible.

Thus the argument which overturns the particular theory of elliptical polarization destroys at the same time all the. other optical theories of M. Cauchy, because they are all built on the principles which we have now *demonstrated* to be false. But though the principles of M. Cauchy are now, for the first time, formally refuted, they were objected to, on general grounds, so long ago as the year 1830, by a person whose opinion, on a question of mechanics, ought to have had considerable weight. This was M. Poisson, who, having deduced from the equations of motion of an elastic solid the consequence that such a body admitted vibrations perpendicular to the direction of their propagation, thought it right to remark that this conclusion could not be supposed to account for transversal vibrations in the theory of light, because (as he expressed himself) "the same equations of motion could not possibly apply to two systems [of molecules] so essentially different from each other" as the ethereal fluid and an elastic solid.*—(See the *Annales de Chimie*, tom. xliv. p. 432). The remark, however, did not meet with much attention from mathematicians, who were, perhaps, not disposed to scrutinize too closely any hypothesis which gave transversal vibrations as a result. Besides, the hypothesis appeared to go much further, as it offered *primâ facie* explanations of a great variety of phenomena; it was one to which calculation could be readily applied, and therefore it naturally found favour with the calculator; and as to M. Poisson's objection, it was easily removed by a change of terms, for when the elastic *solid* was called an " elastic *system*," there was no longer anything startling in the announcement that the motions of the ether are those of such a system. The hypothesis was therefore embraced by a great number of writers in every part of Europe, who reproduced, each in his own way, the results of M. Cauchy, though sometimes with considerable modifications. Every day saw some new investigation purely analytical—some new mathematical research uncontrolled by a single physical conception—put forward as a "mechanical theory" of double refraction, of circular polarization, of dispersion, of absorption; until at length the Journals of Science and Transactions of Societies were filled with a great mass of unmeaning formulas. This state of things was partly occasioned by the great number of " disposable" constants entering into the differential equations of M. Cauchy and their integrals; for it was easy to introduce, among the constants, such relations as would lead to any desired conclusion ; and this method was frequently adopted by M. Cauchy himself. Thus, in his theory of double (or rather triple) refraction, given in the works already cited (p. 145), he supposes three out of his nine constants to vanish, and assumes, among the other six, three very strange and improbable relations, by means of which each of the principal sections of his wave-surface (considering only two out of its three sheets) is reduced to the circle and ellipse of Fresnel's law; and the three principal sections being thus *forced* to coincide, it would not be very surprising if the two sheets were found to coincide in every part with the wave-surface of Fresnel. The coincidence, however, is only approximate; but M. Cauchy is so far from being embarrassed by this circumstance, that he does not hesitate to regard his own theory as rigorously true, and that of Fresnel as bearing to it, in point of accuracy, the same relation which the elliptical theory of the planets, in the system of the world, bears to that of gravitation (*Mémoires de l'Institut*, tom. x. p. 313). Nor is he at all embarrassed by the supernumerary ray belonging to the third sheet of his wave-surface; he assumes at once that such a ray exists, though it was never seen, and promises, for the satisfaction of philosophers, to make known the means of ascertaining its existence (*Ibid.* p. 305). But he afterwards contented himself with observing that as its vibrations are in the direction of propagation they probably make no impression on the eye, and he then gave it the name of the "invisible ray." (*Nouveaux Exercices*, p. 40).

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* As the theory of M. Cauchy (*Mem. de l'Institut*, tom. x.) had been communicated to the Academy of Sciences some months before the period (October, 1830) at which M. Poisson wrote, there can be no doubt that M. Poisson's remark was directed against that theory, though he did not expressly mention it.

In these investigations, the suppositions which M. Cauchy had made respecting the constants led to the result that the vibrations of a polarized ray are *parallel* to its plane of polarization; but in the year 1836 he changed his opinion on this point, and then, by reinstating the constants that he had before supposed to vanish, and establishing proper relations amongst them and the rest, he arrived at the conclusion that the vibrations are *perpendicular* to the plane of polarization (*Comptes Rendus*, Tom. ii. p. 342). All his other results, of course, underwent some corresponding change; and it is this new theory which must now be regarded as rigorous, while that of Fresnel is to be looked on as approximate. But it is needless to say, that if the accuracy of Fresnel's law of double refraction is to be disputed, it must be on much better grounds than these; and the results of M. Cauchy are certainly too far removed from that law to have any chance of being consonant with truth. Although, for example, his new views respecting the direction of the vibrations agree, in a general way, with those of Fresnel, there is yet, in one particular, an important difference between them; for according to Fresnel, the vibrations are always exactly in the surface of the wave, while, according to M. Cauchy (in his old theory as well as the new), they are only so in ordinary media. In a biaxal crystal he finds—and this is one of the ways in which the "invisible ray" manifests its influence—that the direction of vibration, in each of the two rays that are visible, is inclined at a certain angle to the wave-plane; but this angle, though small, is by no means inconsiderable, as M. Cauchy seems to intimate, overlooking the fact, which appears from his own equations, that it is of the same order of magnitude as the quantities on which the double refraction depends. It is true, the deviation measured by this angle cannot, if it exists, be directly observed in the refracted light; but its indirect effects on *reflected* light ought to be very great, since the action of the crystal on a ray reflected at its surface differs from that of an ordinary medium by a quantity of the same order merely as the aforesaid angle; and as the problem of crystalline reflexion has been already solved (Trans. R. I. A. vol. xviii., p. 31) on the supposition (which is an essential one in the solution) that the vibrations are *exactly* in the plane of the wave, it is highly improbable, considering the complex nature of the question, that it will be solved, in any satisfactory way, on a supposition so different as that which is required by the theory of M. Cauchy. However, as the laws of such reflexion are now well known, by means of the solution alluded to, it is possible that M. Cauchy may, as in the case of double refraction, succeed in deducing the same laws, or, if not the same, what may seem to be more exact laws, from certain principles* of his own, helped out, if need be, by proper relations among his constants; especially if, to allow greater scope for such relations, the number of constants be increased by the hypothesis of two coexisting systems of molecules, an hypothesis which M. Cauchy has already considered with his usual generality, but without making any precise application of it. (*Exercices d'Analyse et de Physique Mathématique*, Tom. i. p. 33.)

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* In applying these principles to the question of reflexion and refraction at the surface of an *ordinary* medium (*Comptes Rendus*, Tom. ii. p. 348), M. Cauchy has arrived at the singular conclusion, that light may be greatly increased by refraction through a prism, at the same time that it is almost totally reflected within it. Supposing the refracting angle of the prism to be very little less than the angle of total reflexion for the substance of which it is composed, a ray incident perpendicularly on one of the faces will emerge making a very small angle with the other face; and as the reflexion at the latter face is nearly total, it is self-evident that the intensity of the emergent light, as compared with that of the incident, must be very small. M. Cauchy, however, finds, by an elaborate analysis, that a prodigious multiplication of light [" *une prodigieuse multiplication de la lumière*"] takes place, the emergent ray being nearly six times more intense than the incident when the prism is made of glass, and nearly nine times when the prism is of diamond. This result was, in a general way, actually verified experimentally by himself and another person; so easy it is, in some cases, to see anything that we expect to see. Had the result been true, it would have been a very brilliant discovery indeed; for then we should have been able, by a simple series of refractions, to convert the feeblest light into one of any intensity we pleased; but the very absurdity of such a supposition should have taught M. Cauchy to distrust both his theory and his experiment. Far from doing so, however, he considers the fact to be perfectly established, and to afford a new argument against the system of emission. "Ici," says he, "un rayon, réfléchi en totalité, est de plus transmis avec accroissement de lumière; ce qui est un nouvel argument contre le système d'émission." The system of emission has at least this advantage, that by no possible error could such a conclusion be deduced from it. For if all the particles of light be reflected, certainly none of them can be refracted.

The truth is, that M. Cauchy mistook the measure of intensity in the hypothesis of undulations, supposing it to be proportional simply to the square of the amplitude of vibration; whereas it is really measured by the *vis viva*, or by that square multiplied by the quantity of ether put in motion, a quantity which in the present case is evanescent, since the corresponding volumes of ether, moved by the ray within in the prism and by the emergent ray, are to each other as the sine of twice the angle of the prism to the sine of twice the very small angle which the emergent ray makes with the second face of the prism. The intensity of the emergent light is therefore very small, as it ought to be, though the amplitude of its vibrations is considerable.

Perhaps one cause why M. Cauchy's views on the subject of double refraction have met with such general acceptance, may be found in the fact, that a theory setting out from the same principles, and leading, by the same relations among constants, to formulas identical in every respect with his earlier results, was advanced independently, and nearly at the same time, by M. Neumann of Königsberg (*Poggendorff's Annals*, vol. xxv. p. 418). A coincidence so remarkable would be looked upon, not unreasonably, as a strong argument in favour of the theory; though it must be allowed that, in the effort to extend the knowledge of any subject, there is a tendency in different minds to adopt the same errors respecting it, as well as the same truths; a fact of which we have seen other examples in the course of the present article.

According to M. Neumann (*ibid.* p. 454), the "third ray," not being perceived as light, must manifest its existence as radiant heat, or as a chemical power, or as some other agent ["*als strahlende Wärme, oder chemisch wirkend, oder als irgend ein anderes Agens*"], and he thinks that the nature of this ray will be more easily investigated, if the laws of *reflexion* shall be deduced from the aforesaid theory. But we have seen that the laws of reflexion are, to all appearance, at variance with the theory, and they take no account whatever of the third ray. Besides, the discoveries which have been made of late years respecting the polarization of radiant heat, and the strong analogies that have been traced between it and light, amount to a demonstration that its vibrations are transversal, and of course essentially different from those of the supposed third ray, which are normal, or nearly so. There is every reason to believe that the vibrations of the chemical rays are also transversal; and we may confidently assert, that the three species of rays—those of light and heat, and the chemical rays,—are produced not only by vibrations of the same medium, but by the same kind of vibrations, propagated with nearly the same velocities. If, therefore, the third ray of MM. Cauchy and Neumann has any existence, it must be referred to " some other agent," the nature of which it is impossible to conjecture.

Enough has now been said to show that the optical theory which we have examined, and which has passed current in the scientific world for a considerable period, is quite inadequate to explain the leading phenomena of light, and that it is based upon principles which are altogether inapplicable to the subject. M. Cauchy states, in the memoir so often quoted (*Mem. de l'Institut*, Tom. x. p. 294), that the first application which he had made of his principles was to the theory of *sound*, and that the formulas which he had deduced from them agreed remarkably well with the experiments of Savart and others on the vibrations of elastic solids. As I have already intimated, it is in the solution of such questions (which, however, have long been familiar to mathematicians) that the fundamental equations of M. Cauchy may be most advantageously employed ; and had he pursued his researches in this direction, his labours would doubtless have been attended with more success, and with greater benefit to science.

II.

On Fresnel's Formula for the Intensity of Reflected Light, with Remarks on Metallic Reflexion.

When Mr. Potter discovered, by experiment, that more light is reflected by a metal at a perpendicular incidence than at any oblique incidence (at least as far as 70°), the fact was looked upon, by himself and others, as contrary to all received theories ; and certainly the universal opinion, up to that time, was, that the intensity of reflexion always increases with the incidence. It may therefore be worth while to remark, that the formula given by Fresnel for reflexion at the surface of a transparent body, though not of course applicable, except in a very rude way, to the case of metals, would yet lead us to expect, for highly refracting bodies as the metals are supposed to be, precisely such a result as that obtained by Mr. Potter. For when the index of refraction exceeds the number $2+\sqrt{3}$, or the tangent of 75°, the expression for the intensity of reflected light will be found to have a *minimum* value at a certain angle of incidence; while for all less values of the refractive index the intensity will be least at the perpendicular incidence.

Let i and i' be the angles of incidence and refraction, and put

$$\mathrm{M}=\frac{\sin i}{\sin i'},\quad \mu=\frac{\cos i}{\cos i'};$$

then if I be the intensity of the reflected light, when common light is incident, Fresnel's expression

$$\mathrm{I}=\tfrac{1}{2}\left\{\frac{\sin^2(i-i')}{\sin^2(i+i')}+\frac{\tan^2(i-i')}{\tan^2(i+i')}\right\},$$

in which the intensity of the incident light is taken for unity, may be put under the form

$$\mathrm{I}=\frac{\left(\frac{1}{\mu}-\mu\right)^2+\left(\frac{1}{\mathrm{M}}-\mathrm{M}\right)^2}{\left(\frac{1}{\mu}+\mu+\frac{1}{\mathrm{M}}+\mathrm{M}\right)^2},$$

which has a minimum value when

$$\mu+\frac{1}{\mu}=\mathrm{M}+\frac{1}{\mathrm{M}}-\frac{8}{\mathrm{M}+\frac{1}{\mathrm{M}}};$$

the value of I being in that case

$$\mathrm{I}=\frac{\left(\mathrm{M}-\frac{1}{\mathrm{M}}\right)^2-4}{2\left(\mathrm{M}-\frac{1}{\mathrm{M}}\right)^2}=\frac{\left(\mathrm{M}+\frac{1}{\mathrm{M}}\right)^2-8}{2\left(\mathrm{M}+\frac{1}{\mathrm{M}}\right)^2-8},$$

and the corresponding angle of incidence being given by the formula

$$\sin i = \frac{\sqrt{M}\,\sqrt[4]{\varepsilon^2-1}}{\varepsilon + \sqrt{\varepsilon^2-1}}, \text{ where } \varepsilon = \tfrac{1}{4}\left(M + \frac{1}{M}\right).$$

Since $\mu + \frac{1}{\mu}$ cannot be less than 2, it is easy to see that, when there is a minimum, $M + \frac{1}{M}$ cannot be less than 4, and therefore M cannot be less than $2 + \sqrt{3}$, or 3.732.

As an example, let $M + \frac{1}{M} = 6$. Then, at a perpendicular incidence, one-half the incident light will be reflected. The minimum will be when $i = 65° 36'$, and at this angle only $\frac{7}{16}$ of the incident light will be reflected. The value here assumed for the refractive index is that which Sir J. Herschel (*Treatise on Light*, Art. 594), assigns to mercury; but if my ideas be correct, it is far too low for that metal.

The only person who supposes that the refractive index of a metal is not a large number, is M. Cauchy. It has always been held as a maxim in optics, that the higher the reflective power of any substance, the higher also is its refractive index. But M. Cauchy completely reverses this maxim; for, as I have elsewhere shown (*Comptes Rendus*, tom. viii. p. 964), it follows from his theory that the most reflective metals are the least refractive, and even that the index of refraction, which for transparent bodies is always greater than unity, may for metals descend far below unity. Thus, according to his formula, the index of refraction for pure silver is the fraction $\frac{1}{4}$, so that the dense body of the silver actually plays the part of a very rare medium with respect to a vacuum. It appears to me that such a result as this is quite sufficient to overturn the theory from which it is derived. The formulas, however, which he gives for the intensity of the reflected light, are identical with the empirical expressions which I had given long before, and are at least approximately true.

In framing my own empirical theory (see *Proceedings*, vol. i. p. 2), two suppositions relative to the value of the refractive index presented themselves. Putting M for the *modulus*, and χ for the *characteristic*, I had to choose between the values $\text{M}\cos\chi$ and $\frac{\text{M}}{\cos\chi}$. The latter value is that which I adopted; the former, which is M. Cauchy's, was rejected because I saw that it would lead to the result above mentioned.

Another result of M. Cauchy's, which he has given twice in the *Comptes Rendus* (tom. ii. p. 428, and tom. viii. p. 965) requires to be noticed. When a polarized ray is reflected by a metal, the phase of its vibration is altered, and if the incidence be oblique, the change of phase is different, according as the light is polarized in the plane of incidence, or in the perpendicular plane. But when the ray is reflected at a perpendicular incidence, it is manifest that the change is a constant quantity, whatever be the plane of polarization. In fact, the distinction between the plane of incidence and the perpendicular plane no longer exists, and the phenomena must be the same in all planes passing through the ray. Yet M. Cauchy, in the two places above quoted, asserts it to be a consequence of his theory, that in this case the alterations of phase are different for two planes of polarization at right angles to each other, and that the difference of the alterations amounts to half an undulation. The same singular hypothesis had been previously made by M. Neumann (*Poggendorff's Annals*, vol. xxvi. p. 90), whom M. Cauchy appears to have followed; but M. Neumann has since admitted it to be erroneous (*Ibid.* vol. xl. p. 513).

Mr. J. Huband Smith communicated to the Academy some particulars connected with the recent discovery of a cairn containing cinerary urns, which appears to have been accompanied by some circumstances not unworthy of notice.

It took place at Loughanmore, in the county of Antrim,